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Comment

Interactive comment on “Greenland Ice Sheet exports labile organic carbon to the Arctic oceans” by E. C. Lawson et al.

E. C. Lawson et al.

j.l.wadham@bris.ac.uk

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Reviewer #1

1. This contribution studies the flux of dissolved and particulate organic carbon from the Greenland Ice Sheet (GrIS). Earlier studies from glaciers elsewhere have shown that the organic carbon exported in melt waters is bioavailable (i.e. can be consumed by heterotrophic microbes). GrIS is the largest “glacial” system and as far as I know, the flux of carbon has not been estimated before. This study includes measurements on bioavailable carbon fractions (carbohydrates and amino acids) and incubation experiments assessing the bioavailability. In this light I find it a novel study and of relevance to the readership. However, a vast amount of information is presented and every detail in the data is discussed. This makes for slow reading and for the overall message, or-

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ganic carbon flux and availability, being overwhelmed. So in summary I find this paper suitable for publication but recommend that it is edited to be more focused on the major findings. Try to trim its length down and delete unnecessary sentences (also applies to the supplementary information).

We take on board the comment of Reviewer 1 regarding the length of the paper and inclusion of “unnecessary sentences” and have thoroughly edited the paper to ensure that the major findings are presented with greater clarity. The major findings are;

- (1) DOC and POC export from large Greenland catchments is highly episodic, reflecting the continual tapping of new carbon stores as the zone of melting expands inland
- (2) Bioavailable DOC originates from the ice sheet surface and bed, POC is sourced from the bed
- (3) High OC fluxes from the GrIS that show an insensitivity to runoff changes

2. Introduction. In general it is adequate as is. Not too long and provides a suitable overview of the topic, organic carbon export from GrIS. Methods, 1. What were the units of the suspended sediment concentration measurements? How were these SSC from the grab samples quantified? Dry weight or ashed? Filter pore size? It is fine to put the details in the Supplementary info. 2.

Suspended sediment concentration (SSC) was measured in units of g L⁻¹ and multiplied by the concentration of OC (in mg) per g of sediment to give the POC value in mg L⁻¹. This was then converted to $\mu\text{M C}$ to be consistent with the units used for DOC (see comment below).

SSC was logged at 5 minute intervals using a turbidity sensor calibrated with 80 samples taken throughout the two seasons (2009-2010). Samples were filtered through 0.45 μm papers which were then dried and weighed in the laboratory, as described in (Cowton et al., 2012).

This information has been added to Supplement Sect 1.1 (field sampling).

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3. Might be a good idea to keep the carbon units in mols or mg rather than mix for DOC and POC data. Makes it easier to compare.

We agree that presenting DOC and POC in the same units would aid comparison and we have now converted POC from mg L⁻¹ to μ M C.

The POC estimates seem to most likely have a large error associated with them. There are several stages in the calculation SSC to turbidity then the subtraction of two large numbers (TC and IC). The lack of a relationship between POC and SSC is probably not surprising, bearing all this in mind. So this results in a first estimate of the POC fluxes, but I wouldn't argue that it is proof that the inorganic to organic ratio for particulates is fixed across the hydrograph.

There are several sources that contribute to the potential error in the POC estimates; 1) determining SSC by turbidity, 2) the subtraction of TC and IC to generate OC, and 3) the low number of samples used for POC determination. The level of uncertainty in deriving SSC by calibrating turbidity measurements with the 80 manual grab samples was ± 6 % (as stated in Supplementary Information section 1, pg 1-20). The accuracy of the TC and IC determinations was 3% and 4%, respectively, which will be carried over into the uncertainty in the POC determinations. This has been communicated more clearly in the text. Finally, the low number ($n = 26$) of samples for POC determination may have introduced error. Ideally we would have determined the POC concentration for each suspended sediment sample we collected which would give a more accurate representation of the true POC export throughout the hydrograph. However, we were limited to 26 samples for POC analysis and base our approximations on this, similar to the work of (Bhatia et al., 2013) who determine mean POC concentrations glacial export from early May ($n = 7$), late May ($n = 10$) and July ($n = 12$).

The greatest uncertainty is likely to be due to the low number of samples. Export from Leverett Glacier has been shown to be highly heterogeneous over time, which may not have been captured fully in the average POC determination we used. For instance,

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the percentage variation between minimum and maximum POC fluxes in 2009 is 36%, and 79% in 2010. We have focussed on this error term and have given an envelope of fluxes (as stated in section 2.4, 16), which we stress is an approximation. The errors associated with 1) and 2) would most likely give values within this envelope.

Further work is needed to determine whether the inorganic to organic ratio for particulates is fixed across the hydrograph and was beyond the means of our research.

4. What delta value did you have for the fluorescence measurements? E.g. the excitation at 280 in your synchronous scan corresponds to an emission at ??? Additionally, why normalise to the max fluorescence in the whole dataset. If you are only interested in using it as a qualitative indicator it is better to normalise to the integral, then plot to compare the different shapes.

The delta value for the fluorescence measurements was 18 nm (as we used an 18 nm offset between monochromators). This has been added into the main manuscript.

The fluorescence spectra were normalized to the sample fluorescence peak spectral maximum to facilitate the comparison of sample fluorophore peak location between samples, following previous methods employed in the glacial literature (Barker et al., 2006). This allowed us to compare the different shapes and use fluorescence as a qualitative indicator.

5. Try to avoid repetition with the supplementary information. 6. Be more to the point. E.g. First two sentences of 2.4

The Supplementary Information has been revised and made more concise to avoid repetition, especially when describing the methods (as in section 2.4).

6. Flux Calculations; essentially do not provide any information. There is no need to explain that flux is the combination of flow and concentration. Etc. . . basically just state whether you used a discharge weighted mean or a linear interpolation approach, or .

We agree that Section 2.4. Flux calculations may not provide any new information

and that this could be incorporated into the section on “Seasonal flux calculations” in the Supplementary Information. However, flux calculations were determined by two methods; one method when samples were collected during the chemical monitoring period, and another method when only discharge data was available. We believe that it is necessary to describe these methods but the Supplementary Information may be a more appropriate location, with a summary sentence in the main manuscript.

7. Results Section 3.1 OK. Section 3.2 line 5. How can POC export have units of concentration? Should it not be mass per time?

This has been corrected to read “Estimates of POC export from Leverett Glacier in 2009 ranged from 1.88–7.10 Gg yr⁻¹ and averaged 4.32 Gg yr⁻¹. This exceeded the range in 2010 (0.28–3.11 Gg yr⁻¹, mean = 1.18 Gg yr⁻¹), despite double the water flux in 2010”.

We no longer make reference to Table 2 when describing the POC fluxes as POC in Table 2 is reported as a concentration.

8. Section 3.2 and 3.3. Try not to bring in discussion into this results section. Just present the data as is. Then in the discussion you can bring in the references and relate to other studies.

We take on board the Reviewers comment and have revised the manuscript to ensure greater clarity and removal of repetition and information that detracts from the major findings. This has included making the results and discussion sections more distinct and avoiding discussion of the results in the results section.

9. Discussion: There are some very long sentences which make for heavy reading. E.g. line 5-10 and 10-14 in Section 4.1

This has been addressed and the sentences have been shortened for greater clarity.

10. Split section 4.2 up. Discuss first what the amino and carb analysis shows, then how the fluorescence backs this up. Discuss up the bioavailability results and how they

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compare with other studies separately

We thank the Reviewer for this comment and have separated section 4.2 into two sections following the Reviewers advice. We have created a sub-heading “Bioavailability and active bacteria growth” that begins on pg 19324, line 9, until the end of that paragraph.

11. Section 4.5 I am unsure how important this carbon flux is for the ocean end member. The estimates you derive may be comparable but you also show that much of it is bioavailable and would be respired before even leaving the fjord systems. I would down play this argument and maybe consider other effects. Increased labile carbon will increase competition between heterotrophs and autotrophs for mineral nutrients and potentially alter coastal food webs. Frede Thingstad et al had a Nature paper on this in 2010 which might be worth looking into for inspiration.

We take on board the Reviewer’s point. It is quite possible that the impact of the DOC exported will be greatest within the fjord systems around the ice sheet. There are parts of the ice sheet (e.g. along the north-west margin between Pituffik and Upernavik, and along most of the south-eastern margin) where fjord systems are small or absent. Hence, in these regions coastal and open ocean effects may be seen. There is currently no data on the fate of OC exported from glaciers within fjord systems. However, we have moderated our arguments along the lines the Reviewer suggests.

12. Figure 2. Legend states that suspended sediments are plotted. You mean the POC data? Why not plot the POC in micromolar so that it is directly comparable with DOC.

We thank the Reviewer for identifying this error. The caption for Figure 2 should read “ Export of major inorganic (total ions and dissolved silica) and organic (DOC, POC and free carbohydrates) compounds in Leverett Glacier bulk runoff in (a) the 2009 melt season, and (b) the 2010 melt season. Total discharge is also plotted”. We have amended this and have plotted POC in units of $\mu\text{M C}$.

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